

Response to Referee #2

August 31, 2026

*The document is color coded in the following way:
Referee comments are in **blue**.
Our answers are in **black**.*

We thank Referee #2 for the time and effort he/she spent reading the manuscript and providing constructive comments. Please find our answers to the comments and our plans for addressing them below.

Main points

Terms like clearly and somewhat are used too much and too loosely. They make the text more subjective and less quantitative.

We have decreased the use of these words in the revised version.

The OSI-SAF data is always the last value in the table or the last plot in the figure, but it is the first mentioned in the text. Rearrange the figures and tables so it comes first.

We have rearranged the tables and figures, and OSI SAF is now at the top. In consequence of the comments from referee #1, we have decided to remove the global models from the analysis.

Minor points

Line 10: Why defined like this “We focus on spring (March-April-May) and autumn (August-September-October) sea ice area”?

Sea ice in the Barents Sea usually reaches its maximum in April and its minimum in September <https://mosj.no/en/indikator/climate/ocean/sea-ice-extent-in-the-barents-sea-and-fram-strait/>. The seasons were defined around the minimum and maximum.

Line 127: ML not defined until line 151.

ML is now defined where it is first introduced.

Line 129: Is April the maximum sea ice? Some years March, some years April.

April is usually the maximum in the Barents Sea. We have added "usually" to the sentence

Line 145-149: Are these the definitions of overestimate and underestimate?

Yes, this is correct. We have made this clearer in the text.

Line 159: Why is $p < 0.1$ chosen? It doesn't inspire confidence.

This was an early choice during the analysis process, so that the hashing in the trend maps would not be too restrictive. Changing it to 0.05 would make the statistically significant areas slightly smaller. We have changed all p-value thresholds to 0.05 in the tables, and added "...." hashing to all figures showing p-values for values between 0.05 and 0.1.

Line 170: Why -3 in equation.

The term $N - 3$ arises from Fisher's asymptotic result that the Fisher-transformed correlation coefficient

$$z = \text{atanh}(r)$$

is approximately normally distributed with variance

$$\text{Var}(z) \approx \frac{1}{N - 3}.$$

Line 194: Make is more clear what the comparison is here: "...clearly overestimating" compared to the observations (OSI-SAF).

We have tried to make this clearer when we rewrote the results section.

Line 207-208: Rephrase. Both show positive and negative phases. I think what you mean is NorESM has high frequency and lower amplitude interannual variability, but it is not clear. If this is what you mean, state the standard deviation.

The CMIP6 models have been completely removed in the revised version.

Line 231-270: This section is too vague and unquantified. Too much use of phrases like "much smaller", "somewhat larger", "rather close to", "relatively close to".

The Sea ice edge section has been completely rewritten.

Line 297: The point positive is really small, the significance here must be marginal. Is it significant at $p > 0.05$?

Yes, it is significant at $p > 0.05$. In the updated Figure 5 (Figure 4 in the revised manuscript), where "... " hashing has been added for p-values between 0.1 and 0.05. Using 0.05, the statistically significant area is slightly smaller.

Line 325-326: These two sentences are contradictory. How is the inner ice pack defined, 80%?

Yes, it is defined as $> 80\%$. We have made this clearer in the text.

Line 446-460: While this is true it is difficult, there is mounting evidence that interannual variability comes from the ocean. Given you discuss the link of mixed layer and sea ice it seems important to have a more in-depth discussion of work that has looked at the topic.

This paragraph has been rewritten and moved into the introduction following the suggestion of Reviewer #1.